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A Case of Variolous Fever, showing the Good Effects of Bloodletting. In a Letter from Dr. THOMAS J. WINDER, *Carthage, Somerset County, Maryland, to Dr.* JOHN R. COXE.

I FORWARD to you, for your Museum, a few facts relative to a case of small-pox; and should they prove the means of arresting the arm of death in similar cases, the intention of the author will be amply repaid. And although they are not of a very recent date, yet they may be no less useful on that account.

In the spring of 1796, I was requested by the venerable Dr. Leven Irving to visit, in company with himself, a Mrs. Harrison, of an emaciated habit, about 55 years of age, of Salisbury, whom he informed me was labouring under a violent fever. When we saw her, we found the fever high, and of an inflammatory kind. It immediately struck me that it was the variolous fever, and I communicated that opinion to the doctor; for it was but a few days before that I discharged a house of inoculation about one mile from town. It was mutually agreed on, that she should be removed instantly out of town to that house, when the next day too well proved that our suspicions were well grounded. An eruption came on, with violent

symptoms of inflamed fauces, and difficult deglutition, &c. The eruption was very thick, and, as the pustules progressed, they sunk and put on that black appearance which Dr. Cullen and his predecessors say evidently indicates a great putrescency and dissolution of the fluids, and for which they recommend the free use of bark, wine, the elix. vit. &c. When the patient was removed, the care of her devolved solely on myself; and, as I was averse to the practice of Dr. Cullen, having seen it tried fully, under favourable circumstances, without any good effect, a few years before that, in the state of Virginia, I therefore adopted the lancet, believing that those symptoms which Dr. Cullen and others believed to depend on a putrefaction of the fluids, indicate the very reverse, and evince, unquestionably, the presence of great inflammatory action. I was led to that opinion by Dr. B. Rush; and, however I differ from that illustrious professor on some points, I am ready to acknowledge my obligations to him for this hint, and many other principles in medicine that I have since made a part of the groundwork of my profession. But how often I bled in this case I cannot charge my memory, but I believe as often as is usual in a common pleurisy. Suffice it to say, that I founded the cure principally on the lancet and glysters, making use of gargles, diaphoretics, and medicines of less consideration, occasionally. The fact was, the pock rose and pustulated on the use of the lancet, and in a very short time I discharged my patient well, and the old woman was living not many weeks ago.

An Essay on Insensible Perspiration, and the supposed Absorption and Emission of Gases by the Surface of the Human Body. *By* JOSEPH KLAPP, *M. D.*

(Continued from page 224.)

IV. ARE gases transpired and inhaled through the surface of the human body?

Many of the experiments which I have to relate under this head of the investigation, are extracted from my Inaugural Essay, which was printed in this city, in the year 1805. My apology for introducing them in this place, is derived from the following considerations.

Some of them have been made, by a very respectable physiologist of London, with different results. On this account, and their important tendency in physiology, I wish to bring them more completely before the public, with the hope of being thereby able to stimulate some third person to ascertain, by repeating our experiments, who has committed the error. If I have been deceived, I certainly shall be thankful for a speedy and liberal correction, as truth is, to me, far more desirable than the retention of such opinions as I may have been induced to hastily and improperly adopt. In justice, however, to myself, it may be proper to state, that my experiments were not privately conducted, but in the presence of several persons conversant with the subject. But the gentlemen who particularly witnessed their performance, and to whom I conceive myself under some obligations for assistance in their execution, are the late Drs. Woodhouse and Hutchinson. From the former gentleman, I was honoured with a written

attestation of the accuracy and impartiality with which my experiments were conducted. I shall take the liberty of introducing this letter, as the reputable character which my lately deceased friend has left behind him, will, I am certain, protect me from all ungenerous suspicions.

May 3, 1805.

DEAR SIR,

It afforded me great satisfaction to witness the zeal and industry with which you conducted the inquiry which has led to the important deductions contained in your Inaugural Dissertation, disproving the existence of an aeriform function in the skin.

With pleasure, I bear witness to the neatness and accuracy with which your experiments were performed, and consider them as completely satisfactory on this interesting subject.

That the light which you have thrown on this question may stimulate you to extend your inquiries to other branches of the science of medicine, and that you may enjoy health and happiness in life, and meet with success in the exercise of your profession, is the ardent wish of,

Dear sir,

Your sincere and affectionate friend,

JAMES WOODHOUSE.

Mr. Joseph Klapp.

Having ascertained the nature of the gases contained in the pump-water of the Pennsylvania hospital, where some of my experiments where first performed, no doubt was entertained but that the inquiry would, in some degree, be promoted, by experimenting with my hand and arm held in a glass vessel

filled, and inverted in it. Accordingly, a few experiments were made in this manner; but, as water has been objected to, it may be as well to withhold them until unexceptionable ones are submitted. It may, however, in this place be mentioned, that the gases collected differed in no considerable degree from those which had been previously obtained from the water, by exposing it to a gradually increased temperature.

To promote the purposes of an inquiry like the present, it appears to me, no just or strongly marked exceptions can be taken to quicksilver, as a proper medium to experiment in. With this belief, the necessary quantity of this metallic fluid was procured, and a tub, with requisite conveniences, arranged for its reception. I am pretty confident, if any airs are thrown out by the skin, the fact can be easily ascertained, by immersing the hand and wrist in quicksilver; and, in case they should be transpired, their kind or nature can afterwards be detected by applying to them suitable chemical tests.

With these views I commenced the course of experiments now to be related.

EXPERIMENT XI.

Fahrenheit's Thermometer 73°.

After all loose atmospheric air which might be adhering to the surface of my hand and wrist was completely separated, by moving them in different directions under the surface of the quicksilver for ten or twelve minutes, they were introduced into a glass vessel which had been filled with and inverted in the quicksilver. In this state I continued for upwards of two hours. During the first few minutes of the immersion, my hand was in some degree benumbed with cold, but its usual

warmth soon returned; and, long before the above time was expired, I was sensible of little or no inconvenience, excepting what proceeded from the pressure of the mercury. Notwithstanding the length of time the immersion was continued, not a single bubble of either carbonic acid, azotic air, or of any other kind could be perceived to emanate from the skin.

This experiment was first instituted by me, about eight months ago, at the Pennsylvania hospital, in the presence of Dr. James Hutchinson, and was, at the present time, repeated in the chemical laboratory, in the presence of professor Woodhouse and others, with precisely the same result.

Lime water is considered a just, and I believe it is a pretty delicate test for carbonic acid; if the latter be excreted by the skin, it must, while the hand and wrist are immersed in the former, produce carbonat of lime, and the milky colour which thereby occurs, will serve as the indication of its transpiration.

EXPERIMENT XII.

Fahrenheit's Thermometer 70°.

My hand and wrist were introduced into a jar containing lime water, whose purity, or readiness to combine with fixed air, had been previously ascertained. The process was continued one hour. No carbonat of lime was formed; on the contrary, the lime water, on withdrawing my hand, was as transparent as it was before used. The passage of atmospheric air into the jar was guarded against, and, in every particular, the experiment was conducted with fairness and precision. Since it was first instituted, I have seen it repeated by different persons; and, in one instance, although the immersion was

continued double the above time, yet the result was the same as in the first trial.

This experiment, then, not only shows fixed air is not transpired by the skin, but it also proves the same thing with regard to other gases which are not possessed of an affinity to combine with lime water. Had any of them been discharged, they would have been seen rising in bubbles through the lime water, but this was not the case.

EXPERIMENT XIII.

Fahrenheit's Thermometer 56°.

Having half filled a convenient vessel with water, which had been boiled until it had parted with all the loose air it contained, my foot and ancle were then introduced into it, and retained in it for three hours. Now, as it is well known that boiled water absorbs carbonic acid with avidity, it will only be necessary, in order to discover whether that acid gas had been discharged by the skin, to find out, by a suitable test, whether the water had acquired any of it during the immersion of my foot and ankle in it; to ascertain which, a glass vessel was filled with, and inverted in it, over a large dish; a handful of fresh leaves, taken from a healthy plant, was then introduced into the inverted vessel, and the whole exposed to the rays of the sun for four hours. During that time, no oxygen air was disengaged from the water; therefore, it is very presumable, fixed air was not emitted by the skin. Every one, I suppose, is fully acquainted with what Dr. Woodhouse and others have established concerning the economy of vegetation, to wit: that the leaves of plants readily detect and decompose, by the aid of solar light, any carbonic acid contained by the water in which they are immersed. In this case, the operation of dou-

ble elective attraction takes place. The fixed air is acted on by the divellent forces of two causes. The plant attracts its carbon for nutrition, and the light combines with the oxygen to form oxygen air.

EXPERIMENT XIV.

Fahrenheit's Thermometer 56°.

Having procured some pure hydrogen air from diluted sulphuric acid and the filings of malleable iron, four ounce measures of it were thrown up into a glass vessel, previously filled and inverted in quicksilver. My hand and wrist, after all loose air was separated from their surface, as in a former experiment, were introduced into the inverted vessel, and retained in this situation for three hours. In this way, I experienced very little inconvenience from either pressure or coldness. In the presence of Dr. Woodhouse, the air in the vessel from which my hand had just been withdrawn, was examined. Having ascertained that the volume of the air had not been diminished, we passed up one ounce measure of it through lime water, in a eudiometer, but no milky appearance was by either of us observed to take place. About three ounces of lime water were then passed up into the vessel containing the remainder of the air; no carbonic acid, however, could be detected in either trial.

The foregoing experiments show, that gases are not *transpired* through the surface of the human body. I shall, in the next place, relate some experiments which have induced me to believe, that airs are not *absorbed* by it.

EXPERIMENT XV.

Fahrenheit's Thermometer 60°.

Five ounce measures of atmospheric air were thrown up into a glass vessel previously filled, and inverted in quicksilver. My hand and wrist, after the necessary precautions, were introduced into the vessel, and retained there with very little inconvenience from the mercury, during three hours. We immediately proceeded to analyze the air out of which my hand and wrist had just been taken: its volume had not been diminished; and, when passed through lime water, no milky colour ensued, which circumstance is an additional proof that the skin does not throw out carbonic acid gas. We next endeavoured to discover whether any of the two gases which compose atmospheric air had been taken up. For this purpose, one ounce measure of it was passed up over water, in a eudiometer charged with a bit of phosphorus; and, in less than twenty-four hours, the absorption of $\frac{22}{100}$ was complete.

This experiment alone clearly proves, that neither oxygen nor azotic air are absorbed through the surface; but, in order to afford every satisfaction on the subject I am able, the following varied ones are submitted.

EXPERIMENT XVI.

Fahrenheit's Thermometer 54°.

Five ounce measures of oxygen air of 4 per cent. purity having been transmitted into an inverted vessel filled with quicksilver, my hand and wrist, with the previous cautions, were introduced into it, and held in it for three hours. Upon examination, after withdrawing my hand, it appeared, that the

quantity of the air had experienced no diminution. It was then passed through lime water, and, as turbidness did not occur, we inferred that carbonic acid had not been given out by the skin. The air was next analyzed, as in the last experiment, with the eudiometer of Berthollet, and was found not in the least adulterated.

In order to ascertain whether or not fixed air be absorbed through the external surface, the following experiment was performed.

EXPERIMENT XVII.

Fahrenheit's Thermometer 61°.

Five ounce measures of carbonic acid, obtained from carbonat of lime, by diluted sulphuric acid, were transmitted into a glass vessel previously filled, and inverted in quicksilver. My hand and wrist were next introduced into it, and kept in this state for three hours. On withdrawing my hand, the volume of the air seemed somewhat diminished; perhaps to the extent of half a drachm; the removal of which, in all probability, was owing to its combination with the perspirable fluid. When the affinity is adverted to which subsists between this acid gas and all matters of an aqueous nature, the occurrence in the experiment which I have mentioned is, perhaps, what ought to have been expected.

The emission of airs by the skin was first contended for by certain authors, who had experimented in common water, without ever attending, or in the least adverting, to the water previously containing the very gases which they collected. The circumstance of different gases, such as oxygen, azote, carbonic acid, &c. being actually contained by common water, or the

most of it, and the disposition of those gases to rise out of the water when acted on by the heat constantly passing out of the skin, places such a construction on their experiments, as to greatly diminish the importance which has long been attached to them. The authors I here allude to are count De Milly, whose paper is recorded in the Memoirs of the Royal Academy of Sciences at Berlin, for the year 1777; Mr. Ingenhouz, whose brief and imperfect experiments may be found in his own works; and Mr. Trouset, a French writer, a translation of whose paper may be seen in one of the volumes of Nicholson's Journal. The two first authors have contended for the transpiration of carbonic acid gas, and the latter denies that this is the case, and adduces experiments to prove it is only azotic air which is emitted.

Dr. Rush used to relate, in his lectures, an experiment which he considered very favourable to the theory of the transpiration of gases by the skin. A lighted candle brought to him in the morning, before he had risen, was suddenly extinguished by introducing it into the air of the bed in which he had slept the preceding night: from which the professor concluded, that the air under the bed-clothes had been contaminated by the emission of either carbonic acid gas, or azotic air, by the skin. I have viewed this experiment in a different light. I can readily subscribe to the fact as related by the doctor, but cannot agree with him in his conclusion from it. The facility with which the process of respiration contaminates atmospheric air, by consuming its oxygen, is familiar to every one; and, in my opinion, the true solution of the experiment is, that the adulteration of the air in the bed was owing to this pulmonary process. This explanation will seem very probable, when it is recollected how liable we are to lie, now

and then, in the course of a night's sleep, with our heads under the bed-clothes.

The following facts, which rest on the foundation of experiments presently to be related, prove, that error is attachable not exclusively to De Milly and Ingenhouz, nor to Trousset, who controverts their experiments, but that all three of them are mistaken.

I. The gases collected by holding the hand and fore-arm in pump or common water, are of the same nature, and are nearly in the same proportions to each other, that they are in the water previous to its use.

II. If a part of the body be immersed in boiled or distilled water, which has been entirely deprived of all its loose air, gases of no description whatever are disengaged by the skin.

III. If the hand and fore-arm are invested with fine varnished silk, which is not penetrable or pervious to air, small bubbles of gases, corresponding with those previously contained by the water in which the experiment is made, will be seen to rise from the surface of the silk.

EXPERIMENT XVIII.

Fahrenheit's Thermometer 70°.

A Florence flask was better than half filled with the pump-water of the Pennsylvania hospital; and, after adapting a syphon to its mouth through a cork, it was placed by the side of a pneumatic tub, and a gradually augmented temperature applied. The extremity of the syphon communicated with a bell

glass filled with water. Globules of air, in a short time, began to rise through the water, and collect in the top of the bell glass. When a sufficient quantity of the air had been collected to admit an examination, with Dr. Hutchinson I proceeded to this process. It appeared to both of us, on subjecting it to the action of the common tests in the eudiometer, that the air consisted of carbonic acid, oxygen, and azote, nearly in the proportions to each other as in that which I had previously obtained by holding my hand and fore-arm in an inverted half gallon bottle filled with water from *the same pump*.

EXPERIMENT XIX.

Fahrenheit's Thermometer 69°.

My hand and fore-arm were completely invested with fine varnished silk, which was impervious to air, and introduced into a tub of the same water as above. In a short time, innumerable small bubbles of air were seen by us both, to form at different places on the surface of the silk, and to gradually rise in succession to the top of the water. These bubbles of air presented themselves, and took their flight through the water, in the same manner as we had before observed them to do when the naked arm was immersed in the water.

At the instigation of Dr. Hutchinson, the following experiment was performed, to show more completely the power of a degree of heat equal to that of the skin, to disengage from common water the gases it is impregnated with.

EXPERIMENT XX.

Fahrenheit's Thermometer 70°.

Water, of the standard temperature of the animal body, was passed, during half an hour, through a tube which was fixed in the centre of a large glass globe, previously filled with water directly from the pump. As the warm water was poured into one end of the glass tube, it immediately passed through the middle of the large globe of water, and was discharged through the other end of the tube on the ground. In a few minutes, small globules of air began to form on the external surface of the tube, which was surrounded with the pump-water of its ordinary temperature, and to rise to the top of the globe.

Dr. Hutchinson, though once a believer in the emission of airs by the skin, on perceiving the bubbles of air rising out of the water from the surface of the tube, as I have mentioned, declared he now saw the deception De Milly and Ingenhouz had laboured under. It would be absurd to contend that the air disengaged came out of the pores of the glass tube, and yet, from what experience I have had on the subject, I am disposed to believe such a conclusion would be as just as it would be to assert, that the air which is collected by holding the hand and fore-arm in common water, is discharged by the cutaneous passages. Any inanimate substances, say a stone, a piece of glass or iron, warmer than the water in which they are placed, will, by expanding the airs in the water, by parting with their greater heat, disengage gases as well as the hand and arm. It has been suggested, that water has been objected to as an improper medium for experimenting in; but this objection only holds good against common water, and not against that

which has been well boiled or distilled. All the common gases, excepting carbonic acid, may be collected nearly as well over boiled water as over quicksilver, or any other way. The boiled water certainly absorbs fixed air with great rapidity, but if fixed air should be emitted by the skin, and be immediately combined with the water, such an occurrence can easily be ascertained, by afterwards applying to the water the usual tests for that air.

EXPERIMENT XXI.

Fahrenheit's Thermometer 64° *in the room.*

On the 12th of March, 1809, I introduced my hand, and the greater part of the fore-arm, into a gallon bottle nearly filled with water, which had been boiled for some time, so as to drive off all its loose air. To intercept the communication between the external air and the inside of the bottle, the varnished silk used in the experiments on insensible perspiration was again applied to the neck of the bottle, and about my elbow. In this state I continued during two hours, without either myself or friend being able to discern a single bubble of any kind of air rising through the water.

In this experiment, no one will pretend that the least difficulty existed to the transpiration of air by the skin, if such an office is really executed by it. The cirumstance of the water having been boiled could not have formed an obstacle; and, I am sure, its temperature was every way conducive to the performance of the natural offices of the skin. On withdrawing my hand, Fahrenheit's thermometer was introduced into the water, when the mercury rose in the tube to 76°. Notwithstanding the common objection to boiled water, I certainly feel safe in concluding, as no air was seen to pass through the

water, none was thrown out by the skin. Admitting the affinity of water for air is increased by having been boiled, yet I cannot think it could be so much strengthened as to incorporate carbonic acid, much less azotic air, with the water, so perfectly, and so directly, as to effectually prevent a single bubble from escaping through the water and being seen. However, that even this last objection shall no longer exist to propagate the least degree of doubt, I will proceed to relate the examination of the water I made after withdrawing my hand and forearm from it. The tests which were applied are,

1. Sulphuric acid,
2. Infusion of litmus,
3. Fresh vegetable leaves.

Lime water is very commonly recommended to detect fixed air in water; but I have not been able to succeed with it as an unequivocal test, having found, when poured into water which had been so much boiled as to dissipate all its loose air, some degree of turbidness ensues; and, if permitted to stand in the vessel an hour or two, a white cloud will present itself at or near the bottom.

I presume, on the two fluids mixing, a small quantity of the lime is precipitated.

Two vials were nearly filled; one with the water I had experimented in, and the other with water from the same pump, which had not been boiled. A small quantity of the sulphuric acid was poured first in the one vial, and then in the other, when copious small globules of air quickly ascended through the fresh water, while in the other no appearance was presented but the mere mixing of the acid with the water. The

infusion of litmus was poured into some of the water in which I had held my hand and fore-arm, and pieces of paper, stained with the litmus, were also introduced into other portions of it; but a red colour, the mark of acidity, was not in either case produced.

In the next place, a small handful of vegetable leaves was introduced into a large tumbler which was filled, and inverted in *the water from which I had just withdrawn my hand and arm*, and a second handful of the same leaves was also introduced into another tumbler which was filled, and inverted *in common pump-water*. These tumblers were exposed, during three hours, at the same time, to the same solar light. They were closely attended to by an assistant and myself. At the close of the time stated, on the top of the common water, we found, that about half an ounce measure of oxygen air was collected, while in the other tumbler, containing the boiled water, in which my hand and arm had been held two hours, only three small globules were perceived, and those adhered to the leaves at the time they were introduced. From the time those globules were seen to rise from the leaves into the top of the tumbler, until the three hours had expired, not *a particle of air* was detached from the water.

From the experiments related under the last department of this investigation, I have concluded, as air is not transpired by the skin while the hand and arm are held in boiled water, or in quicksilver, and as the hand and wrist do not contaminate pure oxygen gas, or atmospheric air, when held in them for hours together, *gases, in all probability, are neither transpired nor absorbed through the surface of the human body.*

Case of Chorea Sancti Viti. In a Letter to the EDITOR *from Dr.* PEACHEY HARRISON.

Harrisonburgh, Rockingham County, Vir. July 15, 1809.

DEAR SIR,

AS you have taken much and very laudable pains to investigate the nature of chorea sancti viti, and to systematize the known facts relative to that disease, I take the liberty of communicating a short notice of a case which has lately fallen under my observation; and I feel the more inclined to submit it to your consideration, because the methodus medendi has, I believe, been conducted upon a plan somewhat novel in the history of chorea; and, perhaps, too, the case may cast some light on the abstruse nature of this disease. But it will be referred to you, sir, to determine whether the method of cure is in correspondence with the theory you have suggested; whether it tends to confirm or subvert it. On this point my mind has not been able to bring itself to any decisive conclusion. The case is this:

Nancy Harrison, aged about 13, of a habit very thin, a complexion pale, delicate, and fair, with eyes prominent, considerable vivacity of spirits, and, in short, with most of the distinctive marks of a highly formed nervous temperament, had, about the 1st of March last, without any previous illness or morbid symptoms of any kind, slight appearances of those peculiar gesticulations which constitute or distinguish the chorea. She had never as yet menstruated, nor had any symptoms, premonitory of that constitutional discharge, been experienced. The symptoms were neither preceded nor accompanied by any pain of the head, back, loins, stomach, or any other part. In short, she had enjoyed her usual health, her

usual vivacity of spirits, and regularity of her mental functions. The muscular agitations were, at first, and for the space of two or three weeks, so slight as to be scarcely discernable; they were, however, from the first, extended to her head and upper and lower extremities, but were most apparent in her left hand. For a considerable time, her mother supposed her gesticulations to be merely affected, and repeatedly admonished her to discontinue them, to no purpose; but, on the contrary, they became towards the last of March so distinctly marked, as strongly to arrest the attention of her parents, and awaken their tenderest solicitudes. The physician who usually prescribes for the family was now consulted, and pronounced, without hesitation, the name of the disease, and began to digest, and even to execute, a plan of cure, founded, I believe, on the theory which you have proposed of that disease. The plan, I believe, was intended to include the various means of depletion which might appear to be best accommodated to the strength of the patient, and the state of the symptoms. But the choreatic gesticulations having now become very strongly marked, the novelty of them, and the great apparent sufferings their daughter underwent, created in the minds of her fond parents such indecision, such irresolution, relative to any plan modestly proposed by the diffidence of a physician well acquainted with the uncertain resources of his art with regard to this disease, that the plan was hardly commenced until it was exchanged for one more confidently proposed by a *good woman*, living at a few miles distance, who, having heard of the afflictions of the family, was so obliging as to resign the cares of domestic life, for a time, and take upon herself the important functions of a physician. She found the patient labouring under precisely such symptoms as she herself had experienced when a girl, and which had yielded to the prescriptions of some eminent physician with whom she had

the happiness to be acquainted; and, indeed, the remedies themselves, which this doctress proposed, were far from being empirical; but the *manner* in which they were to be taken was somewhat so. The cold bath was to be used twice a day, in some certain form, which we shall omit to detail. One or two doses a day of the barks, and every third day three pills of asafœtida, containing from six to nine grains. This method was adopted and pursued, without much variation or remission, for five or six weeks; the muscular agitations, however, became, upon the whole, stronger, more confirmed, and more distressing, except that after the adoption of these remedies it was believed that her sleep was less disturbed. But at no other time than when asleep, was there one moment of intermission in the gesticulations; and these were so violent as to shuffle out a pair of shoes in a little time, to loosen her teeth, and to make it necessary to fix soft cushions in the palms of her hands, to preserve them from injury. It was with the utmost difficulty she could take food, and became entirely unable to walk, owing to the extent and strength of the spasmodic contractions of the muscles of the lower extremities; articulation, from the irregular motions of the tongue and under jaw, became also very indistinct.

The case had now assumed a very serious and even alarming aspect, and called for speedy assistance if that were to be had. The attending physician proposed to try other remedies than those which had been tried so long in vain; but, not being proposed with that bold effrontery which alone can overcome that apathy and irresolution which some minds unhappily possess, they were never submitted to, but the whole routine of neighbourhood panaceas, some of which were shocking to common sense, and a scandal to human reason, were eagerly adopted.

This was the state of things, when, some time in the last of May, information was received, through a very confidential channel, that a certain Dr. Sullensteine*, of chivalrous fame

* This Dr. Sullensteine assumes to himself the name of the Turkey doctor, and seems to plume himself greatly on the circumstance of his having been, as he says, bred in Turkey. He seems to think, or wishes it to be thought of him (and in this he shows himself to be an adept in human nature), that this circumstance is greatly favourable to the opinion of his having medical skill. We know what a magical effect antiquity has on all human labours, and that remoteness of *place* has nearly the same deceptive influence on the opinions of mankind with remoteness of *time*. Whatever has been transacted in a distant time or place, has, with men, a greater interest and a greater value. Hence we read with greater avidity the history of ancient and distant nations than that of modern times; hence, too, we are disposed to set a higher value upon the products of the arts and sciences of other nations than upon those of our own. From this principle in human nature, perhaps, springs also the popular opinion, that the art of medicine is more successfully practised amongst remote, barbarous, and savage nations, than amongst ourselves. Hence those itinerant doctors, who have professed to derive their skill from the North American savages, have been greatly resorted to, for a time at least, and the wonders they have wrought have been propagated, with great zeal, through extensive sections of country. Indeed, it seems to be a universal disposition of human nature, to view with a regard somewhat superstitious, whatever is too remote for the investigation of our senses, or too dark for the comprehension of reason.

Of this principle Dr. Sullensteine very successfully avails himself, by laying the scene of his medical education at a greater distance, and in a country more barbarous. It is more than probable, however, that he was never in Turkey, much less bred there. His dialect appears to be that of a Prussian; but, whatever has been his country, it is very certain he never received a medical education any where: but we will do him the justice to allow, that he possesses those qualities which render him pre-eminently fitted for empirical practice: a body at once active and patient of labour; a mind destitute of all science; but, in the same degree that it is ignorant, it is bold, assuming, confident; discerning enough, however, to know when to conceal his ignorance, and when to set the multitude agape by a pompous recital of the many wonders the art of medicine has achieved when directed by his unerring hand. He seems to

in medicine, possessed great skill in the management of all kinds of *fits*, and especially of that form exhibited in this case. Now, as the skill of all the *christian* doctors in the neighbourhood had been exhausted in this case, it could not be blameworthy to call in the strength of even a *mussulman* to subdue this gigantic disease. Dr. Sullensteine is sent for; he comes;

take a pride in undertaking whatever diseases other physicians have declined, or have not succeeded in curing. In short, no disease is so difficult or so inveterate, but he will confidently undertake its cure: and he always attempts to accomplish his undertakings with the *Sampsons* of the materia medica, administered with a bold hand, and with much indifference whether life or death is the result: and it cannot be denied, and it is what every physician would expect, that he sometimes effects great cures. He is one of those doctors who can pursue, with the scalpel, proteiform disease into its most retired situations. Consumptions, so fame announces, have yielded, without a struggle, to the dissecting knife in his hand, in the extirpation of large portions of diseased liver! and, in a case, the subject of which lives in my own neighbourhood, and of which I had the narrative from her own mouth, he was enabled, by superior craft and circumvention, to *catch* and extirpate a living monster, that had for a long time kept its retreat under the knee-pan of the unhappy subject, but which occasionally sallied out of its hiding-place, and ventured a small distance up the thigh. This monster, fell as the hydra of Lerna, had, for a considerable time, eluded the consummate skill and address of Drs. Huter and Miller, as famous in the annals of medicine of our country, as Don Quixote and his squire formerly were in the annals of chivalry: whenever it was found abroad from its lurking-place, before they had the good fortune to seize upon it, it had escaped their grasp. Dr. Sullensteine was, however, better versed in chirurgical tactics, and took the miscreant monster by a coup de main. Its appearance, was, however, too horrid for public inspection; there was no knowing what mischief it would do if permitted to go at large: it was thought most prudent that it be overwhelmed in the flames! But, although, previously to the operation, this medical chevalier had inspired the patient and her father with the utmost confidence in the art, which he solemnly averred he possessed, of *staunching* blood, yet the hemorrhage went on in spite of all his arts, until evanescent life quivered on the pallid lips of the patient! from mere spoliation of blood, life hung in doubtful suspense for several weeks!

"That's just a swatch o' Hornbook's way."

he examines the patient; the case is quite familiar, and no doubt is entertained but it will yield to his remedies in about fifteen days. His plan is proposed; there is some hesitation as to its adoption; it is altogether immaterial to him. If they do not choose to submit to his plan, after putting him to so much trouble, and with a certain prospect of a cure, upon the event of submission, he could return home again.

You may easily guess that he was permitted to go on. Seven blisters are prepared by mixing the powdered flies with fermenting leaven. One is applied between the shoulders, two on each arm, and one on each leg, all pretty large and strong; insomuch that large vesications were produced in about eight hours; but the size of the vesications were not determined by the size of the blisters: for the agitations of the patient rendered it impracticable to confine them (especially that between the shoulders) to a particular place.

Your imagination can supply you with a conception of the patient's sufferings, or rather tortures, during the operations of the blisters, better than any description of mine. Suffice it to say, that the tortures of the vesicatories had nearly dislodged the immortal tenant!

The evening of the next day she had blood let from the foot, to a small amount, and ordered to be bled in a few days again, but this was neglected; was to take some aloetic pills every few days, but these did not operate; took twenty drops of camphorated spirits thrice a day; and, after the vesications dried up, her limbs were to be embrocated with a liniment composed of sweet oil and spirits of turpentine, *with a few red ants*. Immediately after the blisters began to discharge freely, the gesticulations began obviously to abate. Her skin,

which had heretofore been dry, and, it was thought, somewhat thickened, now became soft and perspirable, and articulation began to be more distinct. The blisters continued open for several days, nor, as it was apprehended, did the symptoms recur upon the drying up of the blisters, but turned gradually more favourable; and, at this time, the gesticulations have nearly ceased, and, in all respects, her health is re-establishing as rapidly as could be expected.

I am, respectfully yours,

PEACHEY HARRISON.

Dr. J. R. Coxe.

A Description of the Birth and Form of a Human Monster.

Communicated by JOSEPH KLAPP, *M. D.*

DEAR SIR,

THE bearer will hand you an account of one of the most curious phenomena which perhaps ever fell under the notice of the American faculty; and, if you should think proper, you may give it a place in one of the future numbers of your Museum. The subject of the communication is a human monster, of which a black woman, in the month of March or April last, was delivered, in some part of Bucks county, in this state. The circumstance fell under my notice about two months subsequent to the occurrence of the birth. So far as I can learn, the parents came to this city at the instigation or advice of some ignorant persons, with a view of making money by exhibiting the offspring as a show. A little experience, however, I believe, served to convince them that their undertaking would be attended with more expence than profit. Promiscuous or common spectators, from whom the

proprietors of the many curious objects of public show chiefly receive encouragement, would, I should suppose, avoid rather than court the observance of such an unpleasant spectacle.

After they had been in the city several days, the husband called on me to visit his wife, and then informed me of his having at home this curious offspring. The brief relation he at this time gave me, was such as could not fail to excite my curiosity: I requested him to return, and I would follow him shortly.

On visiting the woman, whom I found much indisposed, her husband placed before me, on the table, unquestionably the most extraordinary lusus naturæ I ever beheld, and what appeared almost equal in singularity to the human monsters which we find described by the marvellous writers of former times. It consisted of two infants, intimately united to each other from the umbilical region up to their head; possessing distinct upper and lower extremities, and a body, neck, and head, common to both children. The head is of an oval figure when viewed laterally, but viewed anteriorly, and including the forehead, it seems somewhat triangular. They are of the same size and length. As they are suspended in the large bottle of spirits of wine, their shoulders, elbows, hands, knees, and feet are placed opposite to each other, and nearly in contact. This disposition of parts is owing to their being applied or united to each other at their breast and anterior part of their abdomen. Their hips and backs are of the usual form, and their bodies are gently inclined towards each other, while their abdomens at the umbilicus unite, and present a smooth uniform surface, covered with a common cuticle. In the centre of the abdomen, common to them both, the funis, in a perpendicular direction, penetrates the cavity so as to be situated at

an equal distance from the proper body of each child. From the umbilical region downwards, they are remarkably well formed; so much so indeed as to give them more appearance of beauty or symmetry, than is commonly perceivable in new-born infants. Their joints are well formed, and their limbs, both upper and lower, with a single exception, are perfectly usual and well proportioned. The exception alluded to consists in there being two thumbs to one of the hands of one of the children. The forehead, and the face generally, is flattened and extended; the malar, or cheek-bones, are further apart than usual. The mouth being open exhibits a perfect tongue and palate. The eye-lids are closed; and, on particular examination, were found to cover well-formed eyes. The general colour of the skin is of a lightish cast; but some parts of it, such as on the hands, the middle of the anterior part of the abdomen, the inner side of the thighs, and the inner side of the arms, the colour is of a reddish brown. The head is covered with a fine kind of dark-coloured hair, not much unlike, or probably would not have been much unlike, that of the mother. Though a coloured woman, yet she has pretty long, distinct, or straight hair, different from the wool of Africans generally. The neck and shoulders are covered with a fine kind of down or fur, of the same colour with that of the head. Under and about the ears, it is much longer and more distinct than at other places. The length of the down or hair is greater about one ear than the other. I have not measured it at this place, but I dare say it is half an inch in length.

The termination of the rectum, and the sexual organs, have a natural appearance. They are both females. On placing the monster in an accurate pair of scales, it appeared that its weight was two pounds and twelve ounces. Its length is four-

teen inches; the breadth of the shoulders is four inches; the length of the arms is six inches and a quarter; and that of the legs six inches. The circumference of the body, at the shoulders, is eleven inches, and that of the head twelve inches and a quarter.

It will be unnecessary to introduce, in this place, a more minute description, as the subjoined plate, containing two of the most interesting views of the object, with explanatory references to the principal parts, will afford ample, and every necessary information. Being informed that Dr. Beatty, of Bucks county, had attended the woman during her confinement, I addressed to him the following letter, to solicit from him such information as his attendance on her had enabled him to collect.

Southwark, June, 1809.

SIR,

As I have not the pleasure of a personal acquaintance with you, it might be considered rather presuming to address a few lines to you on any familiar or common occasion; but, as the purport of my letter is of a nature interesting to science, I trust you will excuse the liberty I have taken with you.

A few days since, a coloured man, lately from your neighbourhood, who calls himself Silas Hunt, left at my house, at my request, a human monster, unquestionably the most singular which has ever transpired to the knowledge of the faculty of this country. So wide a departure from the common form could never have been anticipated, though past experience had supplied us with many more instances than it has done, of Nature's sportive power. He says his wife is the mother of

this production, and that you had the goodness to attend her during the indisposition which followed her delivery.

My object, then, is to solicit from you such information relative to the circumstance, as your attendance on the woman may have enabled you to obtain. I have conversed with her frequently concerning it, and she has given me much curious and interesting information; yet your attestation and account of the event is, notwithstanding, very desirable. I have had a very accurate drawing made of the monster, and propose, at some convenient time, perhaps in the course of a few months, to publish a brief relation of the fact, with the representation of the object.

I assure you, sir, any account which you may find convenient and agreeable to furnish me with, will be very thankfully received.

With respect, I am
Your humble servant,
JOSEPH KLAPP.

Dr. Reading Beatty.

To this letter I was favoured with the following reply, which I have ventured to insert verbatim, in the present communication, without having asked permission from its author. I beg he will excuse the freedom, as I am convinced the interesting matter it contains, is much better detailed in his own language than it would have been in mine.

SIR,

I ought to apologize to you for the apparent neglect your letter has received. I assure you, it was entirely unintentional. From the time your communication arrived, I have been con-

siderably engaged in professional duties, which has left me little leisure for affording you the desired information. Another motive for delay was, that I wished to have some conversation with the female who assisted Sylvia Hunt at the time of her delivery. She is not a professed midwife, but has occasionally afforded her services in that way.

The amount of her information is, that, while the husband was gone for the nearest physician, Sylvia's pains having increased much in force and frequency, she thought it necessary to examine her situation. The membranes were still whole, but through them, in the intervals of pain, she could feel one of the hands. As soon as the water was discharged, she again examined; and, finding the arm likely to present, she, with a considerable degree of resolution, and some judgment, put back the arm, and searched for the feet. In doing this, she found more feet than could belong to one child; and, concluding there must be twins, she endeavoured to keep back one set of feet while she drew forward the other. But finding that drawing upon what she supposed one child invariably brought on the other, she determined to find the cause; and, extending the hand still further, she discovered the junction. She then grasped the whole of the feet in her hand, brought them down, and happily freed the mother from her burden. She says that the monster was born alive, and breathed at intervals, she thinks as long as fifteen minutes.

Some days after delivery, I cannot say how long, I was sent for. I found her labouring under a considerable degree of fever, and a greater discharge of the lochia than was customary. In a few days, however, she was relieved from these symptoms, and I think that in a period not much longer than usual

she recovered. This, I think, includes the whole information I can give you respecting this uncommon case.

Sylvia Hunt has been occasionally under my care for more than three years. The complaint to which she has been most subject, was a uterine hæmorrhage. The discharge was, at times, very profuse, and always tedious, and with difficulty yielded to medicine. The common applications, both internal and external, were resorted to, particularly the sacchar. saturn. which was pretty freely given; and although the relief was not immediate, yet the good effects were very visible. The last time of my attendance upon her in that complaint, was in the month of September last; and, to the best of my recollection (for I have kept no minute of the case), she was not entirely free from it before the latter end of the month. She was delivered of the monster, I think, some time in April, by which it appears that she probably carried it only seven months.

One circumstance more respecting Sylvia, which is rather out of the common road of such things, I must relate.

In February, 1808, I attended upon her during one of her labours, when she was delivered of twins. The child first born, from the colour of the skin and hair, was such as might be expected from such parents. The second child was extremely small, came into the world entirely enveloped in the membranes, and did not see the light till I released the little prisoner. This last child was considerably different from the former, had a much fairer skin, and the hair light and straight. Both the children died when but a few days old.

I am much pleased to hear that you have had an accurate drawing made of the monster, and shall be much gratified in

seeing the draught, and perusing your publication respecting it. When I visited the mother they had put the object into a pitcher, and covered it with spirits. From the size of the pitcher the child could not be readily seen, and I had a very imperfect view of it.

I am sorry that the information given above is not more complete and satisfactory than I am afraid you will find it. Such as it is, I am happy in communicating it to you.

Your's, very respectfully,

READING BEATTY.

Fallsington, August 12, 1809.

DESCRIPTION OF THE PLATE.

Fig. 1. Affords an accurate, and the most interesting view of the monster.

A. A. The lateral protuberances of the common head, apparently formed by the occipital and posterior portions of the parietal bones, manifesting an original effort of nature to construct two distinct heads. This idea derives support from the opposite direction the hair lies in from the centre or middle point of each convex projection; some of it running over the crown of the head, and some towards the neck.

B. The forehead, very much flattened.

C. The face, of an unusual length.

D. D. The cheek bones, placed at an unnatural distance from each other.

E. A portion of the funis umbilicalis included in a ligature.

F. A depression of the common integuments between the anterior parts of the children. Whether there is a sternum for each chest or a common one to both, could not be positively ascertained by an external examination with the fingers; but I am inclined to suspect, there may be an imperfect and separate substitute for a sternum in both cases.

G. Represents the long hair which is spread over the neck and shoulders. This hair is longer about the neck, and near the ears, than at any other place.

H. H. H. H. H, &c. Exhibit the joints and limbs in their usual state, and well proportioned.

I. I. The eye-lids closed, and covering well-formed organs of vision.

Fig. 2. Shows the figure and appearance of the head, from a posterior or lateral view.

A. A long aperture or sinus which proceeds into the head.

B. B. Cartilaginous and fleshy substances which bear a great resemblance to ears.

C. C. The neck and shoulders.

D. The umbilical cord.

E. Representing two thumbs on one of the hands, both perfect in joints and form.

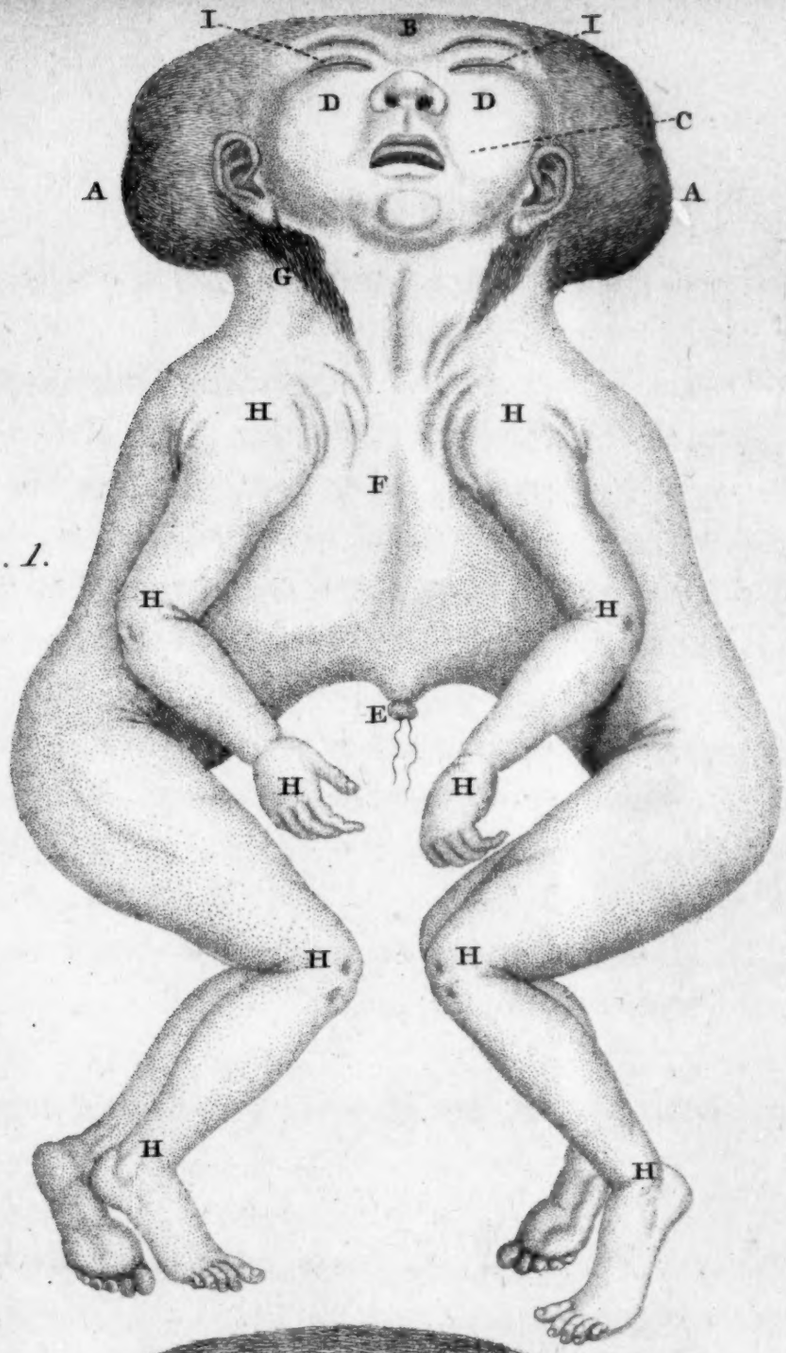

Fig . 1.

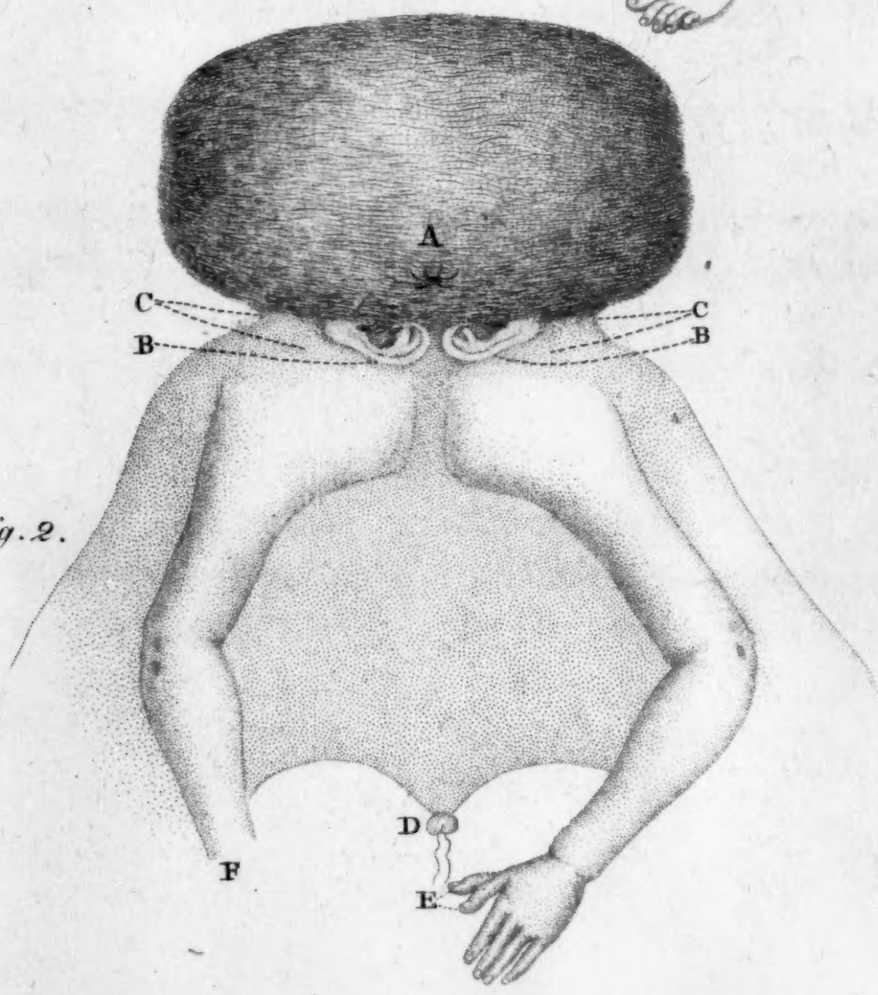

Fig . 2.

F. A part of the fore-arm and hand, omitted, as not necessary to be delineated.

On the Principle of Animation in the Human Body, by THOMAS D. MITCHELL. *In a Letter to the* EDITOR.

DEAR SIR,

THE following imperfect observations were occasioned by the perusal of an Inaugural Dissertation on the Principle of Animation, by Dr. Macrery, of Delaware. If the few remarks I am about to make be worthy your attention, you will oblige me by inserting them in the Medical Museum under your direction.

As all nature owes its origin to some being, some great first cause, which created all things, it is of little consequence by what name we characterize this great being, this infinite power. If we style him God, the great first cause, the alpha and omega, or the governor of the universe, we intend to signify the same personage, the creator and preserver of all things, the almighty controller of events. Then, as all things owe their existence to this great power, they can only be considered as effects derived from this great cause. Therefore, every event in nature, every preternatural occurrence in the course of things, be the instrument which exerts them what it may, the grand principle of action can spring only from this first cause.

My intention here is to consider the principle of animation in the human body; the reader may have already anticipated the conclusion which follows: those who believe that an infi-

nite power created the world, cannot long hesitate to acknowledge the ability of that power to form the creature man. Though this be the noblest of the works of God, yet it excels the rest only in quality, not in degree. Then, if man be a created being, his powers are undoubtedly under the controul of his Creator; and, if man be thus subjected, then is the principle of life which supports him wholly derived from his Creator.

But some are ready to argue that this kind of reasoning is unphilosophical. I am ready to grant that the reception of this animating spark does not come to us from the hand of the Creator in a visible manner; but, although this be true, will any one deny the existence of an animating principle? and, if there be an animating principle, if we are sensible of its existence, shall we deny that it is received from the Creator, merely because he chuses to grant it in a mysterious manner? If we declare as false, things which are really true, because we cannot comprehend them, we might at once deny the existence of man: but sound logic teaches us otherwise. The principle of animation, the vital spark, is the gift of the great first Cause indirectly, through the medium of a wonderful agent pervading universal nature, and known by the different names of oxygen gas, dephlogisticated air, vital air, &c. This is the great instrument by which the wise Creator chuses to effect the principle of animation in the human body; and this, together with its *modus operandi*, shall now claim our attention. One of the distinguishing proofs of the existence of a first Cause, is afforded by the manner in which this vivifying principle is distributed through the universe: no clime is destitute of it, and all share its blessings nearly in an equal degree. It is unnecessary to speak of the manner in which it is afforded; we know it is diffused far and wide, and every living being shares

its bounty. Our business is to attempt to explain the manner in which it operates as a living principle in the human body, and is productive of irritability in it. To suppose a body capable of irritability*, we must first consider that body to be possessed of life; therefore life precedes irritability, and oxygen, being the principle of animation, is, consequently, the source of irritability: this we shall endeavour to prove.

Place a body in a quantity of the air of the atmosphere deprived of oxygen, and it will certainly die. What is the cause of its death? It is deprived of the principle which before animated it. It is of no importance here to examine the changes in the body in consequence of the absence of the vital principle; it is sufficient to know that its absence occasioned death. The body is now deprived of its former irritability, and the reason is obvious: that principle which was the cause of irritability now ceases to act; and, of course, where there is no exciting cause, there is no effect produced. This certainly proves that the presence of oxygen is necessary to life. But some have objected to oxygen as the cause of irritability. Dr. Macrery is among the number of those, and, in his dissertation, he asks, whether the effect produced in the living body by stimuli applied, such as pieces of broken glass, points of pins, &c. is owing to a combination of the oxygen of the body with these stimuli; and if so, in what manner do they unite? We do not pretend to assert that a chemical affinity takes place between the oxygen of the body and the various stimuli that may be applied, nor would we wish to account for this circumstance in any other way than through the medium of actual cause and effect. We have already shown that a body with-

* By irritability in the body we mean, throughout this essay, a sensibility of pain, arising from stimuli applied.

out oyygen will die, and that death and irritability in the same body cannot be; and, therefore, that a body to be sensible to stimuli applied, must be possessed of life, and consequently of oxygen; so that oxygen is the cause of irritability. It is granted that oxygen does not act directly with the stimuli applied to the body so as to produce irritability; but this does not argue that oxygen is not the efficient cause of that irritability; the action of oxygen is through a medium, and that medium is the body. By the action of this principle, the body acted upon is rendered capable of irritability; and, as it was before said, where this principle does not act, its effects cannot follow. When violent stimuli are applied to the body, these act as the remote cause, oxygen as the proximate cause; injury of the part is the remote effect, and sensation of the injury, or irritability, is the proximate effect.

Dr. Macrery further asks, does volition combine with the oxygen of our muscles when we chuse to walk? The doctor certainly was not aware of the objections which might be adduced against his proposition, when he wrote as he did. Are not the muscles exerted when volition or the will is not called into action? Does not the raving maniac walk with as much ease as he who enjoys the blessings of sense and reason? The will is a passive instrument in this case; or, if it were an agent in the action of walking, why would not the decrepid and lame, whose intellectual faculties are unimpaired, travel with as much facility as those who enjoy health and regularity in form and stature? Will the doctor declare that the action of the muscles cannot be exerted, independent of the agency of the will? He, himself, is a living witness to the contrary; therefore his proposition involves nothing of importance with respect to the effects of oxygen in the human body.

Authors have said, that if oxygen be the cause of irritability, how is it that irritability is so completely marked in the fœtal state, where the action of oxygen is so little exerted? The being in the womb, from its first appearance there, until birth, being so intimately connected with its mother, must derive its life from her, through the medium of the oxygen which she imbibes. That intimacy which subsists between the embryo and its mother, renders it a part of the mother, and of course dependent on her for subsistence while in its present state. If the oxygen of the parent be the cause of her irritability (which has been proven already), and as every part of the body is equally liable to irritability, in consequence of the diffusion of oxygen, so the child, being a part of the parent, must partake of the irritability of its parent, and, of course, must be under the effects of oxygen; and, on account of the extreme delicacy of its texture, a proportionally lesser degree of oxygen is capable of producing a proportionally greater degree of irritability than in the mother; and this is the reason why, in the fœtal state, irritability is greater, though the quantity of oxygen imbibed be less.

Finally, oxygen has been objected to as the principle of animation in the human body, because that, notwithstanding its abundance, death is inevitable. The limits of this essay prevent that enlarged view which we might otherwise take of this subject. All know that death is certain. It is a known law of Nature, and of Nature's God, that all must die: but, as the principle of animation was bestowed by the first Cause, through a medium, so also is it removed through a medium. This is effected in various ways, as in suicide, sudden death, intemperance, by chance or design, and protracted severe illness: all these lead to one conclusion. Because oxygen is the principle of animation, does it of course follow that this princi-

ple cannot be counteracted in its effects? If a flourishing tree be cut down as fast as it gains a considerable height, will the vegetating principle which nourishes it be able to raise it above that height? may not the tree be completely cut down and so destroyed that the principle of vegetation cannot restore it? Thus in the human body. If the system be, by any means whatever, disorganized or depraved, it follows, that the force of the action of the animating principle is destroyed, that it cannot exert its full effect. Therefore, the greater the depravation or disorganization of the system (whether suddenly or progressively), by so much the more is the animating principle counteracted in its effects; this principle of action continuing the same, and the system of re-action decreasing until death.

I am yours, &c.

THOMAS D. MITCHELL.

Dr. J. R. Coxe.

On the Method of Preparing and Administering Mercurial Gas in Phthisis Pulmonalis, by Dr. P. W. LITTLE. *In a Letter to the* EDITOR.

Harrisburg, September 13th, 1809.

SIR,

INCLOSED, I send you a communication from Dr. Peter W. Little, on the method of preparing and administering the mercurial gas in phthisis pulmonalis. It is necessary to observe, that these observations were drawn from the doctor in consequence of my writing him on the subject, and requesting the contained information. My wish to be informed was excited by the notice which Dr. Little gave of his administering the mercurial vapour, in his letter to Dr. Rush, which appeared in the 5th volume of the Philadelphia Medical Mu-

seum. I feel it my duty to apologize to the doctor for my negligence in forwarding his communication, and depriving the public of his ingenious and useful observations. My omission did not arise from any want of respect for the importance of the subject, but I wished to forward, at the same time, a case of phthisis which came under my own notice; the successful issue of which I owe, in a considerable degree, to the above-mentioned letter to Dr. Rush. My engagements have prevented me, heretofore, from the attainment of that object; but I have now the pleasure of accompanying the inclosed with the case, the destination of which is submitted to your sound discretion by, sir,

Your humble servant,

SAMUEL AGNEW.

Dr. John R. Coxe.

Mercersburg, May 20th, 1809.

DEAR SIR,

I am very sorry it was not in my power to answer your letter of the 17th April, with which I was honoured, sooner; but be assured, sir, that any information I am capable of giving relative to that now prevalent disease (phthisis pulmonalis), shall be communicated with a great deal of pleasure and satisfaction. At the time I had the honour of making the communication to our learned professor Rush, I had (and still have) it in contemplation to make some experiments, with a view to discover a more ready and perfect mode of administering mercury in a state of vapour or gas than I had then made use of; consequently omitted mentioning the process used. But, from my necessary attention to an extensive and laborious practice, I have never yet, to my satisfaction, been able to complete the experiments contemplated. But as you requested me to inform you the process used to administer mercury in a

state of volatilization or gas, and as I may not for some time have an opportunity of improving this method of administering mercury; and further, as you have a phthisical patient under your care, upon whom you will have an opportunity of improving and perfecting the method, I shall describe the imperfect process used in the cases communicated to the doctor; and, should it prove equally successful with you, and save the life of but one unhappy individual affected with that volcanic disease, I shall feel myself amply compensated for all the anxiety and diffidence with which I make this communication.

Hydrargyrus, in a degree of heat in which it would rise easily in vapour, imbibes pure air and becomes a red calx. This aptitude (if I may be allowed the expression) of mercury to imbibe *air*, and thereby becoming a calx, appeared at first an insuperable obstacle to this mode of administering it; but, upon further reflection, I was led to believe, that if mercury was triturated with a substance equally volatile, and reduced to extreme division of its globules, thereby exposing a greater surface to the action of heat, it might be conveyed into the lungs in a state of vapour: I therefore triturated hydrargyrus with gum. myrrhæ, until its globules entirely disappeared, and had the following machine made for administering it, viz.: a hand-stove made of sheet-iron, about 7 inches square and 5 high, in the top-plate of which I had a circular hole cut large enough to admit a tin of 10 inches in circumference, and 4 high. In this I placed a phial surrounded with sand. To the mouth of the phial I had a flexible tube fixed, about 6 inches long, and fitted with an ivory mouth-piece at the other end. This tube was fitted on the mouth of the phial, so as to be taken off at pleasure. This flexible tube is made after the manner of that described by Bell of Mudge's inhaler. The sides of the stove were perforated with holes, in order to ad-

mit air to keep up the fire, which is made of charcoal. To this stove I had four feet and a handle fixed, for convenience of standing and handling. Hydrargyrus may also be combined with gum olibanum, benzoin, Venice turpentine; or, instead of these, cinnabar may be used, and conveyed into the lungs by using the above machine. The vapour or smoke arising from cinnabar thrown upon live coals, has long since been recommended by Dr. Turner and others, as a very useful and efficacious remedy for drying and healing venereal ulcers and sores, particularly those of the nose, mouth, and throat. A solution of cor. sub. of mercury, in water, may be used with great advantage, by inhaling the vapour when heated*. Mudge's inhaler would be the most convenient, but, if that cannot be had, a tin cup and funnel may be substituted.

I direct my patients to inhale the vapour frequently in the 24 hours when I wish to salivate with it, and when their mouth becomes affected, to use it less frequently. I have frequently thought that we but too often fail in perfecting a cure in this disease, by not keeping up the ptyalism long enough to restore the healthy action of the lungs. I therefore always advise my patients to keep up the effects of the mercury for a considerable time, but in a moderate degree. Calomel may also be conveyed into the lungs by triturating it with myrrh, olibanum, benzoin, &c. Precipitate, and almost every preparation of mercury, by triturating it with a substance equally volatile, and using the described machine, &c. The uncertainty of the dose may, with some other objections, be brought against this mode of exhibiting mercury. But are not all doses of this herculean remedy, given even in the usual and

* It is presumable that any advantage arising from this, must be solely derived from the aqueous vapours, as corrosive sublimate does not rise with this heat; and hence water alone may be substituted.—Editor.

long-customary forms, in some degree uncertain? How frequently do we find but a single dose of 10 grains act as a vomit, a purge, and produce a salivation; when, at another time, in the same person, 200 grains, given at different times, will not salivate!

The effects of mercury upon the human body are now so well known and familiar to every practitioner of medicine, that I hope the uncertainty of dose will not be urged to the general adoption of this mode of giving it in consumption. You ask, have I an opinion that mercury may be introduced into the system in this way when it fails in other ways? or that it will more effectually relieve in this than the usual plan? Few subjects in the science of medicine have excited more attention, or a greater degree of controversy among physicians, than the modus operandi of mercury upon the human body. A question so interesting, and of such importance, in a practical point of view, could not fail exciting attention; and accordingly it has been the subject of much theoretical dispute and speculation. Almost all, however, who have written upon this subject, have agreed in considering it a stimulant, and acting specifically upon the glandular system, in whatever form applied. Consequently, if we consider the extent of the glandular system exposed to its immediate and direct action, when received by the mouth in a state of volatilization, we must necessarily conclude that it is the readiest way of exciting the system into action, and that, when we consider the associated actions subsisting between the vessels of the mouth and those of the stomach, &c. it will not be so liable to run off by the bowels as when given in the usual way.

My worthy and ingenious friend, Dr. Elijah Griffiths, of Philadelphia, told me, that when he failed exciting a ptyalism by giving mercury in the usual way, he always suc-

ceeded in producing a speedy salivation by making his patients breathe the vapour arising from cinnabar thrown upon live coals. In the fourth volume of the Edinburgh Medical Essays, Mr. Hill mentions a case, where the vapour arising from only half a dram of cinnabar drawn up into the mouth and nose, raised a salivation in the space of three hours. We are also told by the same gentleman (as mentioned in my letter to Dr. Coxe, and by him published in the Medical Museum), of a salivation being excited in three hours, by inhaling the mercurial vapour which arose from some mercury which was suffered to stand upon a heated stove with a view of purifying it. We have also the authority of Chaptal, of a salivation being produced merely by inhaling oxygen gas, which was obtained from red precip. From my own experience, and the above facts, there remains no doubt in my mind, but mercury may be introduced into the system in this way when it fails in other ways, and the system affected and excited into action with more certainty, and quicker, than any way yet proposed and administered. That it will more effectually relieve in this than the usual plan, I have also no doubt, particularly when we consider that the lungs are the seat of disease; and that mercury, thus administered, acts as a topical application to the part diseased. From the situation of the lungs, we have no way of applying topical remedies to them, but in a state of volatilization; and I have already mentioned, that the vapour of cinnabar has been used with much advantage in drying and healing venereal sores and ulcers situated in the nose, mouth, and throat. Would not this salutary effect extend to ulcers, &c. of the lungs, if properly conveyed into them. Physicians, until very lately, paid too little attention to this mode of applying medicine to the lungs in a diseased and consumptive state. The beneficial effects of topical applications to ulcers, &c. not kindly disposed to heal, are daily experienced; and, as there is no way (as already observed) of ap-

plying external applications to the ulcers, &c. of the lungs, than in a state of volatilization, it must appear obvious, that it will more effectually relieve in this than the usual plan of treating the consumption.

The difficulty, and the cause of our failing but too frequently, in curing pulmonary consumption, arises first, from the constant action of the lungs. The pernicious and injurious effects of walking, &c. in ulcers of the legs, &c. are daily evinced. But as the constant action of the lungs is indispensable with life, this can never be remedied. Secondly, from the constant admission and action of atmospheric air upon the parts diseased. Hitherto, too little attention has been paid to the temperature and air of the rooms of those labouring under phthisis pulmonalis. The insalutary effects of atmospheric air in external (obvious) wounds, are daily experienced, and too well known to you to require proof. It is also well known to you, that if large ulcers are exposed but for a short time to air, a hectic fever ensues. The action and admission of air to the ulcers of the lungs, must be equally pernicious, insalutary, and the cause of the hectic state of the consumptive. Do not the hectic symptoms always succeed the purulent and ulcerated discharge from the lungs? I have considered the due regulation of the temperature and air of the apartments of consumptive patients, of primary importance in the treatment of that disease, and as meriting our constant attention. The temperature of the room ought always to be such as is agreeable to the feelings of the patient, and as uniform as possible. Impregnating the atmosphere of the room of consumptive patients with mercury, will be found of great service. Hence I cannot but observe, that I disapprove of consumptive patients performing journies on horseback, until the healthy action of the lungs is restored; because they are then exposed to all the vicissitudes

of weather and change of air. In doing this I am fully aware I oppose the opinions of most authors who have written upon this subject, particularly the immortal Sydenham, who says that riding on horseback will as certainly cure the consumption as bark the intermittent fever. Few instances, however, confirm this opinion; and, where it has been of service, I believe it to be only in those cases in which extreme debility of the system continued after the healthy action of the lungs was restored, or where the lungs were never ulcerated. For exercise, I always direct the use of the swing and the rocking chair, until the lungs have assumed their healthy action. I then advise exercise on horseback, increasing the distance with the increasing strength of the patient: but only in pleasant days, and when the temperature of the atmosphere is dry and uniform. As fire has a tendency to rarify air and render it more salutary and agreeable to phthisical patients, I generally recommend the use of fire, and confining themselves to the room in damp weather and days during the summer season.

As I have already far exceeded the limits I set to this imperfect and now prolix letter, I shall merely observe, that the patients alluded to were evidently in the last stage of phthisis pulmonalis. They were much emaciated, pulse small and frequent, beating from 120 to 130 in a minute; febrile exacerbations, profuse night sweats, and a great discharge of thick pus, with extreme difficult and laborious breathing.

The medicines used were principally mercury, digital. opium, episp. &c. prescribed according to the symptoms and state of the system. I have never found any medicine act so powerfully and beneficial in restraining the profuse night sweats as the following preparation: Take sulph. of iron, acet. of

lead, ana ℥ij. French brandy 1 ℔. digest for 24 hours, &c.; of this I gave from 5 to 40 drops twice a day. I have generally added some laudanum at night. I was led to prescribe sac. saturni from its powerful effects in checking hæmorrhages, and I was happy to find it answer my most sanguine expectations. In the place of salt of iron, ipecacuanha may be added with great advantage, so as to excite a slight nausea, &c.

I hope, my dear sir, you will pardon the prolixity of this letter. Should you think it worthy a place in the Medical Museum, you are at liberty to forward it to Dr. Coxe. I do not by any means consider the process used to administer mercury in a state of vapour or volatilization as complete or perfected; it is not yet matured, and but an imperfect child. All I expect, or can hope for, is, that it may prove an exciting cause to some one more capable of suggesting a better and more perfect method of exhibiting it in a state of gas.

I remain,

Your friend and humble servant,

PETER W. LITTLE.

Dr. Samuel Agnew.

A Case of Phthisis Pulmonalis successfully treated by the Acetate of Lead and Digitalis. By Dr. SAMUEL AGNEW.

THE history of the present case is given as it was committed to my common-place book during my attendance on the patient, J. T. a young man about 25 years of age, of a florid complexion.

He had been affected, about the beginning of November, 1808, with a quartan fever, which continued several weeks,

but did not prevent him from his usual labour the intermediate days. Having a strong opposition to the use of medicine, he neglected himself, under the delusive hope that the fever would soon leave him. At length, finding no prospect of relief, he was induced, by some of his good neighbours, to take a celebrated (and no doubt a useful) nostrum for the ague, viz. a composition of bark, snake-root, and crem. tartar. I had previously induced him to be bled and take an emetic. The medicine checked the fever, but did not produce that complete restoration to strength which was necessary to secure a continuance of health. However, he remained free of any marked state of fever during the greatest part of the winter.

On the 16th of April, 1809, he complained to me of bad health, and requested something to be done. He alleged, he had received his present indisposition from a strain in making a violent exertion at lifting a heavy weight, 5 or 6 days previously. He complained of a severe pain across his abdomen and lumbar region, general weakness, some pain in the head, and irregular fever. I took some blood, applied a strengthening plaster, and prescribed the use of bals. copaib. Under this treatment he became better, in a few days, of the pain in the abdomen and back, with some acquisition of strength: but his breast now became, unfortunately, the seat of disease. He complained of some pain in the breast, hard dry cough, an irregular fever, with exacerbations and want of appetite.

On the 20th, I gave him a cathartic, and some antimonial powders to follow the use of the purgative, and restricted him in his diet. The medicines operated well: complained much of the antimonial powder producing severe sickness. He felt

easier ; pain abated, and fever lessened : still some cough, and complains of great debility.

On the 23d, I gave him some of the tr. cinchon. compos. under the hopes that the inflammatory action was pretty well subdued, and that the present symptoms might result from relaxation and want of energy in the absorbent system of the organs of respiration. I was not apprehensive, at this time, that any thing serious would ensue, supposing it to be a mild pneumonia or catarrhal fever, which was prevalent, and therefore was not so decided in my practice as I otherwise would have been ; and what contributed to my relaxation, was his extreme aversion to medicine, and a persuasion that it was nothing more than a bad cold.

He complained now little of any thing but the cough, which was very troublesome, particularly at night. He called on the 29th, and wished some paregoric to relieve his cough ; for he thought if that was easier he would be well. I replied, no doubt the great object was to subdue the cough, but I was apprehensive it would require some other treatment. However, I indulged him with the paregoric.

May 1, Finding himself no better, but that the cough increased in violence, that there was a great stricture in his breast, dispnœa, chills, and fevers, which exacerbated in the evening, and night sweats, with great debility, I stated to him the indispensable necessity of pursuing some other plan for the recovery of his health, as he was threatened with an an alarming disease. He consented to take medicine. I prescribed a dose of neutral salts, and applied a large vesicatory over his breast.

2. The medicine operated well, and the blister rose finely. Felt something easier in his breast. Gave him a pectoral mixture of ipecac. g. arab. Tr. opii and pure water, to be repeated in the quantity of a spoonful frequently through the day.

3. The symptoms not alleviated. The stricture and difficulty of breathing are great—cough very severe—fever continues. Gave him powders, composed of nitre and ipecac. to be used in such a manner as would excite nausea, and produce a diaphoresis.

4. The powders sickened him so excessively, he discontinued them, after using two or three. Not much change.

5. There is evidently an affection of his lungs. He has chills, fevers, and night sweats; his cough severe, and expectoration very copious, perhaps to the amount of a pint during the night. Stricture of the breast great, loss of breath upon the least exercise, want of appetite, flushing of the cheeks, &c. To-day I found it would be indispensably necessary to adopt some more decisive plan, in order to check the rapid progress this fatal foe to human happiness was making, in destroying the all-important organs of respiration. I was well aware of the perplexity in which every feeling mind must be involved, when attempting to digest a plan of attack, which would insure success against an enemy the most insidious and unmanageable that afflicts humanity. I was well assured of the futility of recommending a mercurial course, in consequence of the impressions the patient had, both as it respected the nature of his disease, and the danger and fatal effects of mercury. In reviewing the various remedies which empiricism or false reasoning have recorded as infallible, I could not refrain from the

gloomy reflection, that as yet all the boasted powers of the healing art had been rendered impotent and lifeless, by the all-destroying canker of phthisis pulmonalis. What should I do? I resolved on trying the combined effects of the acetate of lead and tr. digitalis. I must confess that I was led to this decision, not only from the general effects ascribed to these medicines on the human system, particularly in this disease, but from the manner in which Dr. Reese speaks of them, and also Dr. Little, as contributing much to subdue the fever.—I applied to-day a pitch plaster over his breast, which was yet tender from the blister, and also one between the shoulders. I likewise gave him the acetate of lead, in pills, to the extent of 2 grs. three times a-day, and in the intervals the tr. digital. to the amount of 10 drops. His pulse, for a considerable time past, has been very frequent, and rather small, though often too tense; but I could not induce him to lose blood.—His pulse generally at this time counted one hundred.

6. No perceptible change. Continue the medicines.

7. The cough somewhat alleviated, pulse not so frequent, and more soft. Continue the medicines, increasing the tr. a drop every day.

8. A change somewhat favourable: pulse better, counts from 90 to 100.

9. The symptoms are lessening perceptibly in severity.—The cough not so troublesome—expectoration not so copious, and the pulse less frequent. Continue.

10. Continues better. Expectoration diminished very much—fever very inconsiderable—pulse nearly natural—rests pretty

well at night—appetite returning. Continue. Increase the sac. saturn. to 3 grs. three times a-day.

11. Continues better.

12. Continues better. No fever of any consequence—no chills, cough abating much—pulse as slow as 70, and natural.

17. Relieved of almost every symptom of disease—appetite good—bowels regular—no fevers or sweats—no cough, unless he walks smartly, or uses much exercise. Continue.

19. Not so well. He exposed himself too much in bad weather. Days cold, and temperature variable. I cannot induce him to wear flannel. Cough much more severe, and stricture considerable in the breast. I took some blood from him to-day—had an inflammatory coat.

20. Felt immediate relief by blood-letting yesterday. Still too much action in the arterial system. Cough troublesome, and appetite not good. Took about 10 oz. more blood.

22. Better. The stricture is nearly removed—the cough not so troublesome—pulse pretty natural. Takes the medicine as usual.

23. Continues better. Walks through town.

31. Continues free of every symptom of disease, except a slight cough occasionally, which is produced by too much exercise. Takes moderate exercise in the garden. Appetite good, and strength increasing fast. Permitted him to discontinue the digitalis. Continue the sac. saturn. a short time longer, with the addition of the nitric acid.

This patient has remained free of every pulmonary symptom ever since the last date, and is now in pretty good health. But it is to be lamented, that we find so frequently verified the affecting truth, that there is no unmixed good allotted man in this terrestrial abode. This patient owed a scene of considerable suffering to the very cause which contributed to snatch him from the abodes of rottenness. In the course of 6 or 7 days after the last-mentioned date, he was attacked with a severe pain in the epigastric region, which extended from side to side. From the nature and symptoms of this affection, I had no hesitation in ascribing it to the action of the lead. I found it obstinate and difficult to relieve; but I finally succeeded in destroying every remnant of the disease. But I trust I am warranted in saying, that this effect of the lead may be avoided by administering it more sparingly, and yet answer the purpose of removing the pulmonary disease. For it may be observed, that I administered it very liberally, and that I increased the quantity after I was assured the disease was yielding, from a desire that the impression should be permanent, and that every opportunity might be given to prevent any future indisposition*.

I have, however, endeavoured faithfully to delineate the case, in every view which I conceived might be of future advantage to the medical world; and in whatever light it may be viewed by them, I shall flatter myself that I have done my duty.

SAMUEL AGNEW.

Harrisburg.

* A query might be made, how far the nitric acid added to the subsequent unpleasant symptoms, for, by this addition, the patient probably was taking a nitrate of lead, and not an acetate. What the effects of nitrate of lead are upon the system, I know not. EDITOR.

Account of Hanah's Sulphur Spring. In a letter from SAMUEL AGNEW, M. D. *to the* EDITOR.

Harrisburg, 13th September, 1809.

SIR,

I INCLOSE you a brief and imperfect account of a visit I made to a mineral in the adjoining county of Cumberland. My object was to endeavour to ascertain, by a few experiments, some of the more evident properties of the water. But being very imperfectly acquainted with chemical experiments, and having but a few hours to stay, I obtained but little satisfaction. I have given a statement of the appearances of the water, and the depositions about the spring, with a few experiments, from which, if you can draw any satisfactory opinion, even of the more prominent qualities of the water, I will thank you much for your communication of them. I have little doubt of its possessing valuable properties, and am desirous that the proprietor, who is in rather embarrassed circumstances, should have it in his power to benefit himself as well as serve the public.

I am, Sir,

Your humble servant,

SAMUEL AGNEW.

Dr. J. R. Coxe.

I visited this water, in company with S. Laird, Esq. and two other gentlemen, on the 20th and 21st May, 1809. It is situated in a narrow valley or recess, formed by the two ends of the North mountain, which appears to be interrupted here, projecting a little past each other. It is about 17 or 18 miles W. something N. of Carlisle, and perhaps 6 miles N. something

E. of Shippensburgh, in the county of Cumberland, Pennsylvania. The place is wild and romantic, well calculated to delight the eye of him who is fond of the grand and awful productions of nature. It is situated on a dry bank, several rods above a small stream, which runs down the valley, and has a S. or S. E. exposure. It is surrounded by timber, and is almost entirely in a state of nature. The soil appears to be composed of clay and a large quantity of quartzose gravel. There are very few rocks in the neighbourhood, and these small, on the surface of the ground. They appear to be formed of quartz and granite, perhaps in a bed of schistus. I was informed by Mr. Hanah, that the rock from which the water issues, is a slate of a dark colour. I could not have access to this, as the spring is enclosed in a hollow gum set in the ground between 2 and 3 feet in depth. A stream issues from an auger hole of about one inch diameter, and is conveyed off by a small trough formed by the excavation of a small sapling.—On my approach to the spring, I discovered a sulphurous smell, better than a rod distant. It is necessary to observe here, that this was an unfavourable time of the season for examining the water, as the spring has been unusually cold, and of late several rains. Mr. Hanah informed us, however, that the weather made very little impression on the quantity of water, but that in the warm season it emitted something of a stronger smell of sulphur.—The water is remarkably clear and transparent, and of an agreeable temperature. In looking into the spring, confined in the gum, the eye is delighted with a variegated tissue of colours, but that of a purple strongly predominates, making the sides of the gum appear like the finest purple silk velvet. This is confined to about two-thirds of the distance from the top to the bottom. The upper part of the gum has a large quantity of a white substance adhering to its sides, and floating with its fibres in the water, resembling small flakes of wool or cotton,

except that, when you catch it with your fingers, it appears to dissolve, or be dissipated, like the pulp of fine white paper, which has been a long time macerated in water. This substance also adheres in large quantities to the sides and bottom of the trough which conveys the water from the spring. Mr. H. informs, that, if removed, it will form in the course of 30 or 36 hours again. By a close inspection of the surface of the spring, it may be observed to be under constant agitation, not general over the surface at the same time, but at different points, forming a kind of undulation, similar to what takes place when a small insect or particle of dirt falls on the surface of a placid stream. This I was willing to ascribe to the escape of carbonic gas. The water is certainly light, a person being able to drink 4 or 5 pints in the course of twenty minutes. Those who have used the water for a short time, can drink it in much larger quantities. It smells and tastes very evidently of sulphur, but not so strong as some other waters I have seen.

EXPERIMENTS.

1. By adding a small quantity of sulphuric acid to some water in a wine glass, a great intestine motion and effervescence ensued, and a white precipitate.—This, I think, detects the existence of *carbonic acid gas.*

2. By adding a solution of carbonate of soda to about an equal quantity of the water, and holding a glass stopper, moistened with muriatic acid, over it, white fumes or vapour was evidently seen.—This is said to detect the existence of an *ammon. salt.*

3. By adding a solution of the acetate of lead, a white cloudy

precipitate or flocculi immediately appears, and the whole after a short time becomes turbid.

4. By adding some nitric acid, it appeared to lessen the sulphurous smell.

5. By writing with a solution of the acetate of lead on white paper, and holding it over some of the water heated, the letters became of a light copper colour, in the course of 15 or 20 minutes.

A number of other experiments were made, but I was unable to draw any conclusion from them. The water is represented to have had a very happy effect on many diseased persons. It had a very powerful and salutary effect on Mr. Laird, who is generally in a very costive state, and of a bilious habit. Its effects were not so evident on the rest. It is a diuretic.

History of the Typhus Petechialis, or the Malignant Petechial or Spotted Fever; as it appeared in Goshen, Connecticut, during the Winter of 1807–8; *with such Remarks as may tend to elucidate its Nature, and to establish the best Method of Cure. By* ELISHA NORTH.

Goshen, February 12th, 1809.

DEAR SIR,

AGREEABLY to your request, I will now communicate to you the result of the knowledge I have acquired respecting the epidemic which prevailed here last winter.

This epidemic first appeared in the town of Winchester (about eight miles distant), in the spring of 1807.

According to the best information I have been able to obtain, one-third of those who had it died: a great mortality.

The physicians, although some of them were skilful in other diseases, knew neither the nature of this nor the method of cure, until, at length, one was cured by sweating.

Sweating was excited by exhibiting a warm tea made of the twigs of that species of fir called hemlock, and applying blocks of the same boiled in water, and wrapped in cloths, and laid into bed near the patient. This practice was first used by a Mrs. Hurlbert upon her own child, without the advice of a physician, or without the knowledge of the disease. Previous to this all had died. After the cure of Mrs. Hurlbert's child, she was active in introducing her method of practice, and some of the physicians readily adopted it. Afterwards fewer died.

From the inquiries I had made with respect to this disease at Winchester (for I had not had an opportunity of seeing it), I was unable to satisfy myself of its nature, or the best method of cure.

At length it appeared in this town. The three first patients died with it, and were buried in one grave. I saw neither of these. One of the first patients I saw was a young woman eighteen years old. She died within twenty-four hours. She died in what I shall call, in the progress of this inquiry, the *sinking state of fever*. Immediately upon the death of this

patient, I adopted the stimulating practice; with what success you will see in the sequel. At this time, patients beginning to thicken upon me, I happened to see a skilful physician, who had seen a few sick with this disease. I informed him of the plan I was pursuing. He cautioned me thus: "Beware of your stimulants, for the disease is an inflammation of the brain." I told him my remedies *appeared* to afford relief, and I should not alter my plan until I had further evidence. Soon after this I received your letter, which tended to confirm me in my plan, and for which I return you my most cordial thanks. Thus much by way of preface.

The symptoms are the following:

Pain of the head, more commonly the back side; slight chills; furred tongue; great prostration of strength, early in the disease; loss of appetite, although less, I have thought, than in other fevers, especially in the lighter cases; sometimes vomiting and purging; distress about the precordia; pains of the limbs, frequently; sometimes a slight cough; pulse generally weak and quick, sometimes full, but never hard, as I have judged. In some cases the pulse is so little removed from health in quickness, although weak, that I have doubted whether such could fairly be said to have fever in the common acceptation of the term; these had, apparently, little or no febrile heat. Others had great heat, and apparently a high fever. Some had a sweating in the progress of the disease.

If I have not deceived myself, almost all had a kind of œdematous feel of the skin, especially about the hands and wrists. Dejection of countenance.

Upon inquiry, almost all would tell you that they had, in the commencement of the complaint, a slight sore-throat, although few would mention it of their own accord. In a few, but very few, however, I have been able to discover aphthæ on the tonsils. In general, they would tell me the soreness was a little lower down in the throat.

In the bad cases, the most distressing symptoms were pain of the head, and universal distress and agony, which would cause children to draw back their heads, and toss and throw about their limbs: these had a constant sighing and quickness of breathing. Some had delirium. It was sometimes low; in others it was a violent mania. Some were comatous.

With regard to the spots or petechiæ, from which the disease appears to take its name, they are by no means a constant or frequent symptom. When they do appear, they are of various sorts. Some have spots in the true skin (they seldom rise up into pimples like other eruptions) resembling flea-bites. One patient was covered all over with such spots, for a number of days. But more commonly you will find only a few scattered on different parts of the body. They are of different grades of colour, from a red to a dark colour. Some resemble a bruise; others appear as though the patient had been struck with a whip. One patient had a little spot upon her arm; she thought it occasioned by the bite of a spider: from this spot arose an erysipelatous inflammation, which extended over her whole fore-arm, of a dark-red colour, which appeared on the point of a mortification. This symptom was removed within twenty-four hours, by the use of bark and wine.

Some have been attended with slight hemorrhages from the nose; others with symptoms resembling hysteria.

The length or duration of the disease has been very various, extending from one week to five or six. Indeed, many have been in a convalescent state much longer.

I have not often observed any thing like a regular crisis, so often observed in other fevers. In a few of the last cases, however, which occurred (in these the fever followed the measles), the fever attacked the patient like the spotted fever, but soon assumed the form of a common typhus, and terminated by a regular crisis. When these cases occurred, the sickly season had extended into the beginning of summer.

In the close of the same summer, the common typhus fever broke out among us, and has been very rife. This serves to corroborate Sydenham's opinion of the varying nature of epidemical diseases.

Although the typhus fever has been evidently very contagious, yet the spotted fever has appeared not to be communicated by contagion.

Death happens in the spotted fever many times suddenly and unexpectedly, and when there is apparently little danger, at least to common observers.

I shall omit describing the sinking state of this fever, until I come to treat of its cure.

Children and young persons are the most liable to this disease, although I have heard of one woman, who was sixty years old, who died with it. Those of a sanguine constitution and feeble habit, are more liable to it than others.

I have been of opinion, that such causes as had a tendency to produce a debilitated and scorbutic habit of body, must be considered as the predisposing. Of these may be reckoned a moist atmosphere, and less nutritious food than usual. Two seasons preceding this epidemic, have been uncommonly wet: so much so that we have had very little corn, less garden vegetables, and bad grain.

An observing miller has told me, that he never ground so much bad rye in his life: this is the common bread-corn of the inhabitants.

The most frequent immediate exciting cause has been obstructed perspiration by cold.

METHOD OF CURE.

The indications of cure appear to be:

1. To evacuate the first passages.
2. To restore the obstructed perspiration.
3. To invigorate the solids.
4. To correct the scorbutic tendency of the humours.
5. To obviate accidental death.

If I am called in the commencement of the complaint, and discover no symptoms immediately alarming, I first give an emetic, commonly of ipecacuanha, then a cathartic of senna,

rheubarb, or some other mild purge. This I do to prepare the stomach and bowels the better to receive other medicines. While this is doing, I apply a blister on the nape of the neck: this will, in ordinary cases, remove the head-ache. I also direct elix. paregoric, or tinct. opii, in sufficient doses to ease pain and remove distress, to be taken two or three times a day, as long as the patient chooses; also gum camphor reduced to powder, with a little spirit and white sugar, four or five grains, once in two or three hours. Elix. vitriol, and native acids, if they are to be had, are also used. Besides these, I direct wine, or spirit of wine, diluted; wine I have generally preferred: of this take from half a pint to a pint daily.

To the above, I frequently add a tea of Virginia snake-root and Peruvian bark. Of this let the patient take, if he be an adult, two ounces, once in three or four hours. Keep the bowels in order. I see that the patient has a supply of fresh air. Keep his feet warm, either with sinapisms or bottles of warm water. Apply blisters occasionally. Order for diet such as the patient's stomach will best bear.

I sometimes, to vary my medicine, order tinct. cort. Huxh. tinct. castor. com.—ess. menth. pip.—spt. lavend. com. &c.

For common drink I direct warm teas: such as sage, pennyroyal, hemlock, and rob of elder, diluted.

Such a kind of course, in ordinary cases, varying occasionally according to circumstances (keeping in mind my general indications), I have found to be sufficient, in time, to effect a cure.

I will now attend to my 5th indication, which is to obviate accidental death. Here I shall premise a few things, to explain what I mean by accidental death.

In this fever, in the malignant intermittent fever, in the yellow fever, in the angina maligna, in the plague, and other fevers, patients sometimes die suddenly and unexpectedly, at or near their commencement; sometimes in their progress. Now, the causes of death, in such cases, may be supposed different from those in which death is produced by a long and violent course of fever; or, if the proximate cause is the same, it may reasonably be supposed, that the chance of obviating the proximate cause of death, by remedies, must be greater in the commencement of fever, than after fever has continued so long as to injure or destroy the organization of the body. That we are very much in the dark respecting the nature of fever, I readily acknowledge. I wish, therefore, to theorize no further than is necessary to convey to you a practical idea respecting the cure of fever. I am little solicitous whether the theory be true in fact, provided I succeed in communicating my thoughts so as to be understood so far as respects the method of cure.

Suppose a man ninety years old to die a natural death; would you not say that the excitability or irritability of his system had been so worn out by the reiterated application of stimuli, which had been applied, during a long life, that it would be in vain to attempt to resuscitate him by the application of fresh stimuli?

Suppose another person, say a child, to have his excitability or irritability suddenly suspended by drowning, so as to be ap-

parently dead; would you not attempt, in this case, to rouse the irritability by the application of fresh stimuli, and thereby restore life?

Suppose a person to have a fever which shall continue to progress so long as to destroy the organization of the body, and to wear out the excitability of the system, and cause death: in this case it would be in vain to resuscitate him.

Suppose another suddenly attacked with fever, and, by the violence of the pain, or excessive fatigue, or other causes, the excitability or irritability is suddenly suspended, and apparent death produced; would there not be some chance, in this case, that the application of stimuli might resuscitate him? Dr. John Rush has given us one case in which this was actually done.

The first may be called the natural death of fever, the second accidental death.

That what is here called accidental death produced by fever, may be frequently prevented by the timely and thorough application of remedies, is a fact of which I have not the least doubt.

Having theorized thus much, I am prepared to proceed with my curative indication in the sinking state of fever.

If I am called to a patient attacked with spotted fever, and find him attended with violent pain of the head, drawing back of the head, tossing about the limbs, sighing, distress for breath, coldness of the extreme parts, sickness at stomach, great distress and agony, or comatous, with an involuntary

running of the tears, lividness of the lips, or with delirium or mania, with a low and feeble pulse, or, perhaps, none at all, I conclude that there is no time to be lost: that he is in danger of dying in a few hours, either by syncope or pulmonary convulsions or otherwise.

I immediately set myself to work upon the patient with stimulants, externally and internally. I put the patient to bed, after having first bathed his feet in warm water, and then apply a blister to the nape of his neck; give tinct. opii, hot brandy-sling, heated wine, warm teas. I also apply hot blocks or brands from the fire, quenched in water, and wrapped in cloths, near the patient, in his bed. Also bottles of warm water, or similar blocks, to his feet. In some of these cases I have given ol. peppermint, essence peppermint, tinct. castor. com.—camphor, &c. taking care not to crowd the stomach so as to excite vomiting. Generally, as soon as the patient grows warm, he is relieved; not always, however, until he sweats. I have known one case in which the patient sweated an hour without relief: in that case I gave a second dose of opium, and the patient soon became easy. In these patients (after this process had relieved them) the fever appears as in other cases, and is to be treated accordingly.

I have always been careful to direct the nurses, if any symptoms of sinking occurred at any stage of the fever, to give some stimulants at such times, as a little wine, &c. By such practice I have not lost a single patient since the one already mentioned. I have treated perhaps one hundred.

I do not suppose the cure depends upon the mere flowing of the sweat, but upon the effect of the stimuli used for exciting

the sweat. Still I consider the sweating as useful, it being a kind of medical thermometer to enable us to know when the degree of stimuli has been carried sufficiently far in the bad cases.

In consequence of these opinions, I have not directed the sweating process in more than one-sixth part of my patients, and perhaps many of these might have done as well without. But, as I have observed no injury to arise from the practice, and knowing the liability of sudden death, I have considered myself as erring (if I have erred at all) on the safe side, when I directed the practice, provided the sweat was not allowed to flow too long.

I shall conclude with a few remarks upon the practice of others.

In Winchester, bleeding was tried. It was thought it hastened the death of some. In other places it has been tried, and with like success. And yet I find Sydenham bled once in his febris nova, which was a similar fever with this. He, however, cautions against repeated bleeding. His practice was a mixed practice: for he used blisters between the shoulders; also anodynes and acids; also bark, where he discovered aphthæ. Sweating he disapproves of. He sweated his patients twenty-four hours. This might do harm, by dissipating the caloric of the system, and weakening the patient. That he sweated his patients in this manner appears from another part of his book, viz.: where he describes the utility of sweating in the epidemic fever, which prevailed the year after the plague.

The physicians who adopted Mrs. Hurlbert's mode of sweating, applied it to every patient, not knowing but there might be some specific virtue in the hemlock tea. This is so

far from being fact, that I do not think it so good as many other teas. It disagrees with some stomachs.

I am decidedly of opinion, this disease requires stimulating remedies: but by this I do not mean that in every case patients must take brandy by quarts, or wine by gallons; for I believe with you, sir, that "it requires as much judgment in using stimulants as it does in reducing the system."

I am, sir, with much respect,

Your obedient and humble servant,

ELISHA NORTH.

Dr. Elijah Munson, Newhaven.

Cases of Bilious Fever produced by Vegetable Miasmata. By Dr. JOHN A. CASEY.

Augusta, Georgia, 1st October, 1809.

DEAR SIR,

A REMARKABLE instance of the influence of putrescent vegetable matter producing a highly inflammatory bilious fever, occurred in this neighbourhood a few weeks ago. A more convincing case I do not recollect to have seen.

It is the practice of our citizens, at least of the wealthy part, to retire to country situations during the summer and fall months. Some have pleasant retreats on the Sand Hills, situated three miles west of the town, amid piney woods, and removed from mills or stagnant waters of any kind. Others retire to plantations in more fertile parts of Columbia, the county adjoining. Mrs. —— had returned early in the summer, to this latter place, while the negroes were at work in clearing land: during the summer, one hundred and fifty acres were cleared. The miasm from this immense quantity of vegetable

decaying matter was wafted by every breeze to the dwelling, which was unprotected from its influence. Twelve in family made this place their summer abode. Towards the latter end of August, they began to sicken, and before the middle of September, every individual was sick. One died, and the cases of all were severe, highly dangerous, and slow in their recovery. Venesection and copious evacuations by the bowels were necessary. The discharges were black, such as take place in yellow fever, and the skin and livid appearances were such exactly as I have observed in yellow fever. Of a few visitors to the above-mentioned place, two were taken ill. The symptoms in all were alike, i. e. a pain in the head, back, and bones, very furred tongue, inflamed eyes; high fever introduced by a considerable fit, and immediately after assuming the remittent form, which, in the convalescent period, was exchanged for a quotidian intermittent, commonly called the ague and fever. Even with generous nursing, a free use of bark and wine, the restoration was very tedious. Of the two visitors who were ill, one died. The symptoms which occurred during the illness of those that died, were the same, excessive thirst, a burning sensation at the stomach, general coldness; several days before dissolution, the extremities felt more like damp marble in a bleak winter's day; after death a yellowness of the skin, with purple streaks and spots, and a speedy disposition to putrefaction. In one case, it commenced almost with the termination of life.

This tragic scene befel two of our most worthy and respectable families. They were of course very sedulously attended to, and though they were watched with the greatest care, one for fifteen, the other for five days, yet not one of the attendants became indisposed. I had forgotten to remark, the family were brought into town, early in their attack, for the more readily procuring medical aid. Respectfully,

Dr. J. R. Coxe. JOHN A. CASEY.